

## **American Neurological Association.**

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The Second Annual Meeting of the American Neurological Association convened in New York City, June 7, 1876, and was called to order at 2 p. m. by the President, Dr. J. S. Jewell, of Chicago.

Present—Drs. Jewell, Webber, Dalton, Miles, Roosa, Hamilton, Emerson, Hammond, Mason, Arnold, Lente, Shaw, Hun, Cross, Beard, Seguin.

On motion it was voted to dispense with the reading of the minutes of the First Annual Session.

Dr. E. C. Seguin, Secretary of the Council, read the Annual Report, which was adopted.

On motion, the Secretaries were authorized to dispose of volumes of the Transactions remaining on hand, at cost price, to such of the members of the Association as might wish to purchase them, reserving twelve copies for the future use of the Society.

### **NOMINATION FOR MEMBERSHIP.**

Dr. J. J. Mason nominated Dr. Eugene Dupuy.

The President appointed the following gentlemen as Committee on Nominations.

Drs. Webber, of Boston; Lente, of Cold Spring, N. Y.; Cross and Emerson, of New York City; and Shaw, of Brooklyn.

There being no further business before the Association, the first paper for the session was read by the President, Dr. J. S. Jewell upon

### **THE STRUCTURE AND FUNCTIONS OF THE GANGLIA UPON THE POSTERIOR ROOTS OF THE SPINAL CORD.**

Dr. Jewell prefaced his paper with limited introductory

remarks, in which the following items were brought forward as having a bearing upon the prosperity of the Association. The members should feel sufficiently interested in their Association to report their best work at its annual sessions. It was recommended; *First*, that the meetings, for many years, should be held in some one of the three or four largest seaboard cities; *Second*, that all business matters, as far as possible, should be referred to the Council, so as not to consume the time of the Association; *Third*, that during the next three or four years, the election of new members, whether active or otherwise, should be refrained from as far as possible; *i. e.*, until the Society had shown, by the character of its work, that it is thoroughly scientific and respectable; *Fourth*, that committees should be appointed whose duty it should be to report at subsequent meetings upon some specific subject.

The Doctor then proceeded to read his paper upon the subject announced. He first considered the gross anatomy of those ganglia, and considered their situation, size, shape, color and consistence. From thence he passed to the consideration of certain questions relating to minute anatomy and function. Prominent among them were:—

What were the peculiarities presented by the nerve cells of which they were composed?

In what direction did their fibres pass?

What are their true function?

The conclusions arrived at by Dr. Jewell were mainly as follows:

The ganglia upon the posterior roots of the spinal nerves contained cells, which were peculiar, as compared with the cells either of the cerebro-spinal axis or of the sympathetic ganglia. They approached more closely those of the sympathetic, but differed from them chiefly in the fact of being much more frequently uni-polar. In the sympathetic ganglia the cells are usually bi-polar. Perhaps in a majority of cases the cells of the posterior ganglia had only a single process, but there were numerous exceptions in the form of apolar or embryonal cells. Both of the processes frequently took their origin upon one side of the cell, and made a partial or even

two turns around it before making their departure. In most instances the processes were furnished with myeline. Occasionally the cells had two processes; one direct, the other spiral; the direct containing myeline throughout, the spiral containing myeline only exceptionally, which could be seen only at some distance from the cell. The direction of the processes, almost without exception, appeared to be towards the periphery.

There was no demonstrable connection between the sensory fibres and the cells of the posterior spinal ganglia. If there is any influence exercised over the sensory root by those ganglia, there must be some connection between them. Upon that point Dr. Jewell believed that he had had considerable light shed by the preparations made by Mr. R. W. Amidon, of New York, who, it would seem, had shown that there might be a connection between the cells of the spinal ganglia and the fibres of the sensory root.

Dr. Jewell farther believed that it had been demonstrated with tolerable certainty that if the sensory roots were divided peripherally to the ganglia, some effects are produced upon the muscles anatomically related thereto. That change consisted in slow atrophy of the muscular fibre, much slower than when the motor root alone was divided, and was not due to a descending neuritis. If the sensory roots were divided internally to the ganglia, no effect whatever upon the muscles would be produced. Whatever the function of the ganglia might be, it appeared to have reference to parts lying outside of the spinal cord, and was independent of it. They exercised a trophic influence over the sensory nerve-fibres, and the doctor believed that it was possible to afford some plausible reason why they were placed outside of the spinal cord; unlike the motor roots, which had their trophic centres in the cord. Dr. Jewell was of the opinion that the fibres in the sensory roots, which had been regarded as conveyers of recurrent sensibility, came from the sympathetic ganglia, and had their trophic centres in those ganglia, and that they belong to the vaso-motor system, and should not be taken as part of the mechanism of recurrent sensibility. There was no such thing as recurrent sensibility as commonly understood. The paper being open for discussion—

Dr. Webber, of Boston, inquired how soon atrophy of the muscles followed division of the sensory roots.

Dr. Jewell replied that he had not been able to determine that point; and that the time had varied from six weeks to two or three months.

Dr. Webber asked whether there was any lack of reflex tonus which might account for the atrophy of the muscles after division of the sensory root.

Dr. Jewell replied that there had sometimes been slight loss of muscular tone, as indicated by lack of response to the electrical current.

Dr. Webber asked what the nature of the change was which took place in the muscles.

Dr. Jewell replied that there was simply diminution in size, and that atrophy of all the tissues found in the muscles occurred, whether properly muscular or not.

Dr. Webber remarked that it had occurred to him that, perhaps, the diminution in size of the muscles noted by Dr. Jewell, might be explained by the loss of exercise dependent upon loss of tonus in the muscle, and that this change was not analogous to the degenerative changes that followed injury to the anterior cornua of the cord. In one instance there was absolute degeneration; in the other simply loss of size.

Dr. Hammond, of New York, brought forward a single pathological circumstance, which might have some relation to the subject under discussion, and that was with reference to the disease he had called "progressive facial atrophy." As far as he was aware, no rational explanation had been given for the phenomena observed in that affection. He had had the opportunity to make histological examinations in one case, and had found marked diminution both in size and length of the muscular fibres, when compared with those taken from the sound side of the face. Might not that disease, then, be due partially or entirely to the fact that the Gasserian ganglion of the fifth pair was involved, according to the doctrine enunciated by Dr. Jewell? He was inclined to that opinion.

Dr. Jewell remarked, with regard to Dr. Webber's inquiry, that animals had been confined in a state of functional inactivity; and the changes found in their muscles compared

with those found subsequent to the production of lesions in those nerves with which they were supplied. The result of the observation had been that degeneration took place, but not atrophy, or, at least, atrophy was not at all well marked. The change which had been noticed after division of the sensory root, was a perceptible diminution in size; but none, or but little, degeneration.

With regard to Dr. Hammond's suggestion, Dr. Jewell was of the opinion that if his observations proved to be correct, some light would be thrown upon the pathology of the affection to which allusion had been made.

Dr. Webber thought that dividing the sensory root and confining the limb were entirely different conditions. A confined limb could not move, it was true, but reflex movements were continually going on, and the muscle gets a good deal of the benefit following normal contraction.

In the other case, there was loss of sensibility which would very likely diminish the unconscious action in the muscles, continually going on, and would place the limb in an entirely different condition, and a different result might be expected.

Dr. E. C. Seguin, of New York, inquired of Dr. Jewell whether there was any diminution of electro-muscular contractility.

Dr. Jewell replied that it had seemed to him there was.

Dr. Seguin remarked that he would apply that statement to the suggestion made by Dr. Hammond. He had been able to satisfy himself that the electro-muscular contractility, both by the galvanic and faradic currents, was preserved intact in progressive facial atrophy; and that finding such reaction would separate the affection from that condition described by Dr. Jewell.

Dr. Seguin further inquired of Dr. Jewell whether any influence upon the vaso-motor system had been noted.

Dr. Jewell replied, frequently so, but apparently having no connection with the progress of the degeneration in the nerve or disorder in the muscle.

The next paper was read by DR. J. C. SHAW, of Brooklyn, on

#### HYSTERICAL TRISMUS.

The chief point of interest in the paper related to the hypo-

dermic injection of sulphate of atropia in the treatment of that affection. The doses employed varied in size from  $\frac{1}{16}$  to  $\frac{1}{8}$  of a grain, according to the effects produced. The doctor recommended its use with the qualification that small doses should be first employed, the effects carefully watched, and their size increased according to the requirements of each case. His case was treated successfully. The largest injection was  $\frac{1}{8}$  of a grain, and the largest quantity of the atropia daily used was  $\frac{1}{4}$  of a grain.

Dr. Shaw's paper being open for discussion, Dr. Seguin referred to the history of the case related by Dr. Shaw, and corroborated the statements made with reference to its nature, obstinacy and cure.

Dr. Seguin related the history of a case of spasmodic torticollis which had been nearly cured by the hypodermic use of atropia. The patient had submitted to a variety of treatment without benefit, including injections of  $\frac{1}{16}$  grain of sulphate of eserine; but marked relief followed the second injection of  $\frac{1}{16}$  of a grain of the sulphate of atropia, and at the end of two weeks only slight spasm was manifested. The spasm appeared in the afternoon and the time for using the injection was to be changed to the afternoon, instead of administering it in the morning, as had previously been done.

Dr. Jewell believed that hypodermic injection for the relief of pain or spasm should be as near as possible to the seat of the difficulty. The doctor directed attention to the beneficial effects attending the use of morphia and atropia combined, in the treatment of cholera-morbus, obstinate vomiting, etc. In the treatment of cholera he had combined strychnine and caffeine with the morphia and atropia.

Dr. Putnam alluded to the good effects recently obtained by the hypodermic injection of cold water for the relief of pain.

Dr. Webber had seen a severe pain made to cease by the simple introduction of a needle into the painful part.

Dr. Hammond in this connection brought up the subject of aqua-puncture—a process by which a fine stream of water was forced against the skin, and pain thereby relieved. So great was the force of this needle-like jet that it could be made to perforate the skin.

Dr. Seguin wished to state that he believed that he had been the first to use hypodermic injections of water for the relief of pain. This was while House Physician to the New York Hospital in 1867, and the observation had been recorded in Brown-Sequard's monograph on Functional Nervous Disorders, published in 1868. In the same place he would beg the Association to allow him to claim that in the same year (1867), he had given the explanation of the mode of action of non-narcotic hypodermic injections. They produced their effect first, by absorption of the active ingredient into the circulation; second, by pressure on the nerves near the seat of injection; third, by producing a degree of counter-irritation. (*vide New York Medical Journal*, 1867, vol. I. p. 402).

Dr. Webber remarked, with reference to the use of atropia in convulsive affections, that he had given it by the mouth with marked benefit. He had in a number of cases, however, noticed that the use of the remedy had been followed by a peculiar sense of weakness in the legs. So great had been the weakness in some instances that the remedy had been discontinued. It had occurred in a case of choreiform twitching of the shoulder where  $\frac{1}{10}$  of a grain of sulphate of atropia had been taken three times a day for about a week.

Dr. N. B. Emerson, of New York, referred to a case of painful paraplegia, depending upon a malignant growth attached to the spinal cord, in which a combination of morphia and atropia was employed; but it became necessary to change the original proportions in the prescription, because the morphia habit was more easily acquired than the habit of taking the atropine.

Dr. J. J. MASON, of New York, presented a well-marked case of

#### HEMI-FACIAL SPASM

occurring in a man *æt.* 30 years. The spasm commenced in the tongue, extended over the left side of the face, was first tonic and then clonic. The attacks had appeared annually, and continued about one week. Each spasm had lasted about one minute and a half, and recurred every four or five minutes during the entire time of the attack. In the intervals between the attacks, the patient, to all appearances, had

been perfectly well, though of late he has noticed numbness in the fingers of the left hand. There was no malarial or syphilitic history. The first attack was in April, 1874.

EVENING SESSION—FIRST DAY.

The Association convened at 8. P. M. and listened to a paper on

PARALYSIS FOLLOWING ACUTE DISEASES,

by Dr. S. G. Webber, of Boston. The paper contained the Doctor's own observations and those made by other observers upon cases where paralysis, partial or complete, had followed acute diseases, such as cerebro-spinal meningitis, typhoid fever, variola, scarlatina, diphtheria, etc. He called attention to paralysis and allied affections not of cerebral origin, and endeavored to refer the clinical phenomena to definite pathological changes. A temporary suspension of function might explain some of the cases, and there might be slight change in structure, which disappeared as quickly as purely functional disturbances.

The following is a brief *résumé* of Dr. Webber's paper. There were many cases in which the sequel was so slight that it was impossible to decide where or what the nervous disturbance was.

There was a large number of cases of paralysis which were more severe and lasting than those; and, if grouped indiscriminately, there seemed to be a lack of order, but if classified, they would be found to fall each under its own proper pathological division.

There were many cases in which the lesion existed in the anterior cornua, because muscular atrophy was present; and there was loss of electro-muscular contractility. In other cases there might be sclerosis of the white columns, which could be determined by the attendance of appropriate symptoms. It was quite possible, in other cases, that there was only neuritis, and then there would be no spinal symptoms.

The paper being open for discussion,

Dr. J. J. Mason remarked that he had been specially interested in the electrical reactions alluded to by Dr. Webber, and inquired if the Doctor had been able to obtain that phase of degenerative reaction where the muscles of the extremities responded to less galvanic power, fewer elements, than was



necessary to excite healthy muscles—the so-called *Entartungs-reaction* of Erb and Eulenberg?

Dr. Webber replied that he had.

Dr. Mason remarked that he had never observed such a phenomenon upon the muscles of the extremities.

Dr. Webber remarked that he had found it in cases where lesion of the peripheral nerves was present, and in one case of infantile paralysis.

Dr. Seguin referred to two cases belonging to the class contemplated by Dr. Webber's paper. One was a case presenting all the external features of progressive muscular atrophy, and occurred in a well-developed and apparently healthy young man after typhoid fever. There were no symptoms whatever, except those referable to mechanical disturbances resulting from wasting of the muscles. Electrical reaction was not thoroughly studied, but sufficiently so to determine that response to the faradic current was absolutely lost in the atrophied muscles. The case passed from under observation.

The second case was externally similar to the first, but had a different history and termination. The paralysis followed cerebro-spinal meningitis, and was soon complete, although the patient was apparently in good health. There was wasting of the muscles, chiefly peripheral, and most marked at the extremities of the limbs. There was absolute loss of electromuscular reaction to the faradic current, but there was preservation of reaction to the galvanic current in the muscles of the forearm, and a trace of reaction in the thenar and hyporthenar eminences. The thighs recovered spontaneously. The treatment consisted in the daily use of the galvanic current. During the second winter of the treatment, the faradic current was employed, because of the improvement in the muscles. Almost complete recovery took place. As a last resort, in order to obtain reaction to the galvanic current in the muscles of the anterior part of the leg, Dr. H. B. Sands, of New York, performed tenotomy of the tendo Achillis and placed the feet in plaster-of-Paris dressings. The idea was to produce relaxation of the anterior tibial muscles, so as to enable them to respond to the action of the current. On the fourth day these muscles did respond to galvanism. From

that time onward there was steady improvement. The Doctor was of the opinion that the lesion was in the anterior horns, and occurred during the spinal fever. Dr. Seguin also mentioned certain paradoxical phenomena observed with reference to electro-muscular reaction:

1. Recovery of voluntary control over the gastrocnemius without return of electro-muscular contractility.

2. Remarkable return of electro-muscular reaction in the anterior tibial muscles without marked voluntary control. The special point of interest in the case was the relaxation of muscles secured by tenotomy in order to carry out the electrical treatment. That was done in 1872, and primarily for that purpose.

Dr. Hammond inquired of Dr. Webber what he supposed the lesion to be which gave rise, for instance, to paralysis following diphtheria, in those cases in which recovery took place?

Dr. Webber replied that there was probably some lesion of the nerves or spinal cord.

Dr. Hammond.—What lesion?

Dr. Webber referred to cases reported by Oertel and others, in which, after diphtheria, micrococci and minute hemorrhages had been found in the spinal cord. Buhl had found micrococci and hemorrhages, which had given rise to pressure upon the spinal nerves, thus producing the paralysis. When the new growth ceased, there might remain sufficient of the new formation to produce permanent pressure upon the nerve. When such new tissue or material was absorbed, the nerve would return to its normal condition. So also in the spinal cord, if there was such lesion, the new products might undergo degeneration and disappear. If there was not extensive change, the paralysis ceased with the removal of the new product, but if too much change had been produced in the organic structure, the paralysis remained permanent. The locality and severity of the lesion regulated the recovery.

Dr. Hammond.—Then the lesion is not amenable to treatment by such medication as we have knowledge of at present?

Dr. Webber.—Probably not. In diphtheritic cases the treatment would check the growth of the micrococci; such as used in the treatment of the diphtheria, would be proper.

Dr. Hammond remarked that he had asked the questions with special reference to a case which had been sent to him by Dr. Brooks, of Cleveland. In that instance, the paralysis followed diphtheria, and was complete. The doctor first entertained the idea that the patient was suffering from congestion of the cord, or acute spinal paralysis supervening upon the diphtheria, and accordingly administered ergot in large doses. The patient, however, grew decidedly worse, and atrophy of the muscles took place rapidly. Sensation was not impaired in the slightest degree. At the end of ten days, a change in treatment was made, and strychnia given in doses of one twenty-fourth of a grain, and gradually increased until one-sixth of a grain was taken three times a day. No physiological reaction was produced by the drug, but improvement was rapid, and the patient was able, at the end of one month, to walk without a cane. The doctor, finally, did not regard the case as one of spinal congestion, nor did he believe that it was one in which diphtheritic deposits had taken place in the spinal cord. He was inclined to regard it as a case not exactly in accord with those reported by Dr. Webber.

Dr. Webber remarked that it was quite possible the ergot had nothing to do with the getting worse of the patient, but that the increase of the difficulty was simply such as was not infrequently seen in that class of cases. In the same sense it might be said that the strychnia had nothing to do with the recovery, but that the patient had got ready to get well, and recovery ensued.

Dr. Seguin inquired with regard to electro-muscular reaction in Dr. Hammond's case.

Dr. Hammond replied that there was no reaction to either current.

Dr. Webber remarked that it must be allowed that if there was diphtheritic deposit in the spinal cord, or about the nerves, when the diphtheria disappeared the nerves might be restored the same as after injury from any other cause.

Dr. Hammond.—Yes; but in his case there was manifestly an affection of the cells of the anterior cornua. "I do not suppose," said the Doctor, "that Dr. Webber wishes to be understood as saying that strychnia is not beneficial in the treatment of paralysis following diphtheria."

Dr. Webber.—No; but that, inasmuch as the ergot may not have been of disadvantage, the strychnia may not have been of advantage in the case alluded to.

THURSDAY, JUNE 8—SECOND DAY.

*Afternoon Session.*

The Association was called to order by the President, Dr. Jewell, and the minutes of the previous meeting read and approved.

Present—Drs. Jewell, Webber, Dalton, Hammond, Hamilton, Putnam, Emerson, Mason, McBride, Kinnicutt, Arnold, Lente, Shaw, Cross, Beard, Van Bibber, Miles, and Seguin.

The Committee on Nominations reported as follows:

*For President*—Dr. J. S. Jewell, of Chicago.

*For Vice-President*—Dr. F. T. Miles, of Baltimore.

*For Second Vice-President*—Dr. R. T. Edes, of Boston.

*For Corresponding Secretary*—Dr. J. J. Mason, of New York.

*For Recording Secretary and Treasurer*—Dr. E. C. Seguin, of New York.

*For Curator*—Dr. J. W. S. Arnold, of New York.

The report of the Committee was accepted, and the Recording Secretary instructed to cast the ballot of the Association in the affirmative for the gentlemen nominated.

Dr. E. C. Seguin, Secretary of the Council, reported that the resignation of Dr. Meredith Clymer had been received, and that the Council recommended its acceptance by the Association.

The report of the Council was accepted and adopted.

The next order of business being the reading of papers, Dr. Hammond presented a communication upon

A HITHERTO UNDESCRIBED FORM OF MUSCULAR INCO-ORDINATION.

The patient was also presented, and was a boy at three years and ten months, apparently in good health; had not been subjected to exhausting disease; had no paralysis, no derangement of sensibility, no spasm, nothing indicating sclerosis of any part of the cord; no hereditary tendency to nervous disease; was perfectly straight when lying down; could sit without any indication of inco-ordination; could

hear well with both ears; could use both hands with freedom when sitting or lying, and he could stand erect when he had his back supported; but the moment he attempted to walk, the head was turned towards the left side, the body stooped to the left, the left hand and arm were held close to the side and breast, and were flexed, while the right was thrown out straight behind the body. The boy could direct his steps, but walked with the body and limbs in the position described. It had been supposed that the difficulty might depend upon irritation from congenital phimosis, and accordingly circumcision had been thoroughly performed by Dr. Sayre, of New York, but the intensity of the phenomena of the affection had gradually increased since the operation.

Dr. A. McL. Hamilton, of New York, remarked that a number of cases, closely resembling the one exhibited, had fallen under his observation, in which adherent prepuce had been supposed to be the cause, but circumcision had not been followed by any benefit whatever.

Dr. Seguin directed attention to the fact that the position assumed by the patient when walking was analogous to that seen in animals when the middle peduncle of the cerebellum and adjoining parts of the brain were injured.

Dr. Dupuy concurred in the opinion given by Dr. Seguin, and believed that there was in the present case, lesion in the peduncle, between the cerebellum and pons; and that as soon as movement was attempted, there was a more marked contraction of the muscles upon one side than upon the other.

Dr. Hammond.—But both arms are affected, the right as well as the left.

Dr. Dupuy replied that the same throwing out of the opposite limb is observed in animals whose lateral peduncle has been wounded.

Dr. Seguin.—But is not the right limb thrown out to balance the body?

Dr. Hammond presented a *second* case, with the following history: The patient was thrown from a wagon four years ago, remained unconscious twelve days with a fracture of the skull near the posterior angle of the right parietal bone. He was paralyzed upon the same side. Two years after he came under

the doctor's observation, and complained of vertigo, pain in the head, and epileptiform convulsions.

He was trephined, and had had no convulsions since, and the cerebral symptoms had disappeared. He came again with the complaint that he was entirely deprived of virile power. He had also some paralysis of the bladder, but that was improving, and there was some cystitis. How were his present symptoms to be explained?

Dr. Putnam.—Was there marked depression at the seat of injury?

Dr. Hammond.—There was, and several loose fragments of bone were removed.

Dr. Seguin.—What was the condition of the bone?

Dr. Hammond.—Healthy; and there was no evidence of disease of the membrane below, except very trifling congestion.

Dr. Dupuy.—Had the fits been noticed to affect one side alone?

Dr. Hammond.—I am not able to answer, because the patient was not under observation.

Dr. Dupuy regarded the case as an important one, and that it was a further illustration of the fact that irritation of the dura-mater with electricity almost invariably gave rise to motion upon the same side. It was properly a case of reflex paralysis.

Dr. Hammond thought the paralysis too permanent to be of a reflex character.

Dr. Dupuy remarked that reflex paralysis might be permanent.

Dr. Jewell.—Does Dr. Hammond think that the cerebral lesion gave rise to the paralysis of the bladder?

Dr. Hammond.—There has been no evidence of lesion of the spinal cord, and I think that the paralysis of the bladder therefore is of cerebral origin.

Dr. Jewell inquired how such a high lesion could produce such a low paralysis?

Dr. Hammond thought it might be explained on the ground that there were spinal nerve-fibres continuous with the cerebrum; and the original injury gave rise to trouble which subsequently produced paralysis of the bladder.

Dr. Miles referred to the demonstration of Budge with reference to continuous nerve fibres from the cord to the brain.

Dr. Lente suggested the possibility of remaining cicatricial irritation.

Dr. Webber suggested that the loss of virile power might be dependent upon lesion of the cord disconnected from the brain lesion; the lesion of the cord not manifesting itself in any other way. The doctor referred to a case in which a man something over sixty years of age had had complete loss of virile power since he was twenty-three years of age, when he was thrown from a carriage, and received only an exceedingly slight injury of the back that gave rise to no special inconvenience, and to no spinal symptoms except the one mentioned.

Dr. Jewell remarked that he had been led to locate the genito-spinal centre at the implantation of the fourth lumbar nerve, as had been first pointed out by Budge. Now, if that were true, by what mechanism could loss of virile power be produced by cerebral lesion? Dr. Jewell was of the opinion that there was some lesion in the lower part of the cord; and the practical question was, Was the lesion primary or secondary to the lesion higher up?

Dr. Hamilton suggested that the lesion in the cord was secondary.

Dr. Jewell regarded it as quite probable.

Dr. Dupuy remarked that Vulpian had demonstrated that no such centre exist as referred to by Dr. Jewell.

Dr. Jewell remarked that experiments had proved to him there is such a centre, for, by irritating that point, slight contractions of the uterus, bladder, and seminal vesicles could be produced.

Dr. Dupuy remarked that he had seen the same contractions produced by irritating the second thoracic ganglia.

Dr. Putnam was unable to understand how descending degeneration could take place without giving rise to other symptoms.

Dr. Beard remarked that loss of virile power and affections of the bladder might be developed without any symptoms

whatever of trouble in the spinal cord. No other symptom being present, we are obliged to assume that the entire trouble was in the brain.

The President called the attention of the Association to two microscopic preparations of Mr. Amidon, showing what appears to be a joining of two nerve fibres at right angles in the posterior spinal ganglia. This arrangement, if verified, would serve to show the connection of ganglionic cells of the posterior ganglia with the spinal cord, and with sensory nerves, thus explaining the trophic influence of these ganglion cells.

Dr. Dupuy remarked that a short time ago Prof. Ranvier had discovered and figured the same mode of junction of nerve fibres in the ganglia, and called them, "*terminaisons en-T*."

The next paper was read by Dr. G. M. Beard, of New York, and was entitled

THE INFLUENCE OF MIND IN THE CAUSATION AND CURE OF DISEASE  
—THE POTENCY OF DEFINITE EXPECTATION.

Dr. Beard maintained that disease might appear and disappear without the influence of any other agency than some kind of emotion. Mental qualities, like drugs, could neutralize therapeutics, and they could also increase the effects of drugs. Fear, terror, anxiety and care, grief, anger, wonder, and expectation were regarded as the most likely to produce disease. When active and unrestrained, those emotions became more effectual in the production of disease and the destruction of health, than the most potent poison. The doctor cited various circumstances, illustrating how disease might be brought on by mental states; and referred to a paper lately read before the New York Academy of Medicine by Dr. Gouverneur M. Smith, upon "The Epidemics of the Century," in which it was claimed that the immunity the people had from the ravages of epidemic diseases was due to improved sanitary measures. Dr. Beard was of the opinion that the escape from such diseases was due a great deal to the fact that there was less apprehension of them; and that that element should be taken into consideration in making up the estimate with reference to the value of any means employed for the prevention and control of contagious and epidemic diseases.

Reason, hope, joy, resolution, ambition, self-confidence, ex-



pectation, were regarded as elements which contributed to the recovery of patients, restoring them from actually existing disease and from death itself, and might neutralize the cause which gave rise to malarial fever and to epidemics. That was a fact long ago recognized, and it had been both used and tremendously abused.

The doctor then gave an outline of a series of experiments which he had performed, for the purpose of testing how much could be done in the way of effecting cures in cases of rheumatism, neuralgic sleeplessness, and various forms of chronic disease, by exciting in the patients a *definite expectation*; for example, telling them that upon a certain day or hour, or even minute in some cases, they were to be better or be relieved of pain.

When such predictions were made, a therapeutic force was set in motion, which acted constantly, day and night; and if, by resolving to do so, any one of us could wake up at a certain hour, we should be able to get rid of disease in the same manner. There was no imagination, in the ordinary sense of the term; in the instances in which Dr. Beard had experimented, for the patients were actually sick and the average results he had obtained were superior to the average results obtained under the other method of treatment, but were not equal to the average results obtained when the two methods, the mental and the medicinal, were combined. The effects which had been brought about through the emotions, had been as permanent as those realized through the agency of medicines or electricity in the same class of patients. It was maintained that in disease, in ninety-nine cases out of a hundred, the emotions were supreme. The knowledge, on the part of the patient, that mental therapeutics was being employed in the treatment of their disease, did not discourage them; and the liability to disappointment in carrying out such therapeutics was no greater than that attending the use of any other remedial measure. Mental therapeutics could be applied to acute and chronic, to functional and organic diseases. As an offset to disapproval of such practice, it was urged that physicians were constantly availing themselves of its assistance, either consciously or unconsciously, but that the great majority used it indiscriminately.

It was not the intention of the author of the paper to discard medicinal or other means commonly employed in the treatment of disease, nor to recommend the substitution of what he had called mental therapeutics for other methods of treatment in common use; but simply to direct attention to the influence which the mind had over the body, whether in health or disease, hoping thereby to stimulate the profession to a more thorough study of the subject, for the purpose of availing themselves of its highest advantages.

While many cases could be cured by mental therapeutics alone, for a majority of cases it could be best used as an adjunct to proper medical means; and when so used, the average results ordinarily obtained in the treatment of disease might be improved by a large percentage.

Dr. Lente raised the question whether the benefits attending the use of hypodermic injections of warm water might not be explained by mental influence equally as well as by pressures and irritation, the usual explanation.

Dr. Hammond remarked that, if the doctrine advanced by Dr. Beard was to be accepted, he should feel like throwing his diploma away and joining the theologians. It was probably applicable in certain cases which had fallen under Dr. Beard's observation, but there was not a tithe of evidence regarding its efficacy for the removal of organic disease. The paper was simply a formulation of results which we have had in our possession for more than a hundred years. That cures had been effected in the manner alluded to was undoubtedly true; but the element of deceit was present, because when a patient was told that he was to be cured simply by mental influence, no success whatever would be obtained. If that form of practice was to be prevalent, it should be done under some other guise than that of the practice of medicine. "If," said Dr. Hammond, "I give a patient a glass of water, and tell him with confidence—though he does not know that he is taking water—that he is to be relieved, it may be justifiable; but if it is done generally and kept up, it must be done under its true colors, and then it will fail."

Dr. Beard remarked he was aware that the method of treatment had been employed for hundreds of years; but the point

he insisted upon was, that the profession did not employ it in a systematic manner. Some had used it with more system than others, and studied special ways of acting upon the patient by such means. The object of the paper was not to show anything new, but to introduce to the profession some special experiments, with the view of exciting a more thorough study of the subject. The point of special interest was, to show how we might utilize definite expectation in the treatment of disease. Does Dr. Hammond admit or deny that warts are organic affections?

Dr. Hammond.—I am not prepared to say. I am not prepared to say but that warts are functional.

Dr. Beard remarked that all he claimed was, that physicians could more certainly help their patients by availing themselves of that method of treatment in conjunction with other methods.

Dr. Hammond remarked that Dr. Beard was not qualified to follow that form of practice; that he was too well informed and too intelligent. It was doubtless true that Dr. Newton had cured his patients; and that patients had been cured by the water of Lourdes; and that cases of King's evil had been cured by the royal touch; but if that form of practice was to be instituted, we should go back to monkery—give up our instruments, give up our medicines and enter a convent.

Dr. Miles, of Baltimore, fully recognized the influence of mind over the body, but regarded the handling of it as far more dangerous than handling the most powerful drugs. To assume to possess a virtue when you had it not, was very dangerous.

Dr. Beard, in reply to Dr. Miles' objection, stated that all of us were brought into the same strait, and met the problem in various ways. We were making use of opium, bromide of potassium, electricity, with the expectation of seeing certain effects produced; but we do not know that opium—

Dr. Hammond.—But we believe it will.

Dr. Beard.—We hope it will.

Dr. Hammond.—No; we have sufficient knowledge concerning the power of opium, that we may speak with certainty concerning its effects.

Dr. Beard.—I give a patient a dose of opium, but I do not know whether it will produce sleep. Opium generally produces sleep, but its action is different upon different persons, and there is a chance that it will fail in the present case. A prediction about the effect to be produced by a dose of bromide of potassium may be fulfilled, or it may not.

Dr. Jewell remarked that there were many things which made it credible that mind had power over the body, and instanced the relief from toothache a person experiences when the dentist's room is reached—an effect unquestionably due to mental influence. Now, while this method of influencing our patients might be carried to excess, it was to be assumed that the honest physician would not do that, and he believed it to be the wish of the author of the paper that the profession should candidly study the question, and endeavor to arrive at a golden mean in the application of this method of treatment. Both extremes should be avoided.

Dr. Putnam had never seen any evidence that cure had been effected by mental influence in cases where actual disease had existed, and did not believe that it was proper to state to our patients what we did not know positively.

Dr. Miles regarded it as making definite promises concerning something of which we had no knowledge; and if the patient could look into our minds, he would become cognizant of the fact that we were deceiving him.

Dr. Webber did not believe that Dr. Beard recommended telling falsehoods to our patients; but it seemed to him that he had approached very close to the border line between truth and deception. Good results, doubtless, could be obtained by the proper use of mental influence, but when we overstepped the truth our profession at once was degraded.

Dr. Putnam regarded the experiments as unscientific, because the emotions could not be isolated.

Dr. Mason objected to the term mental therapeutics, and denied its existence.

Dr. Emerson regarded the experiments as unscientific, because the exact nature of the elements experimented with was not known.

Dr. Hammond believed that if the ideas of Dr. Beard were

adopted, we should be descending to the level of all sorts of humbuggery; and he hoped that the paper would not go to the public with the endorsement of the Society.

Dr. Beard regarded the remarks of Dr. Hammond somewhat in the light of a caricature; and claimed there was nothing in his paper which implied that we should throw aside our medicines and resort to humbuggery. The object of the paper was simply to call attention to a special means of using the mind in the treatment of disease—namely, producing definite expectation; but it was not to supplant other methods, or turn the profession into a body of quacks. A further object was to stimulate systematic study of an agent which was daily being employed in the treatment of disease.

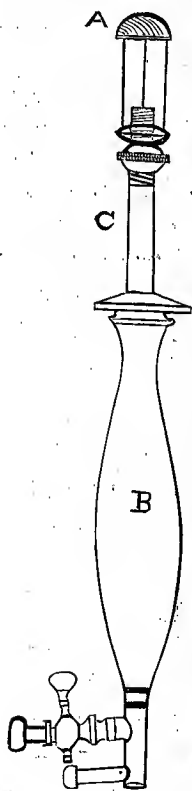
Dr. Denny, of Boston, upon invitation, exhibited a series of transverse sections through the human brain, intended to illustrate its minute microscopic anatomy. The

brain from which the sections were made had been undergoing the hardening process, by means of alcohol and bichromate of potash, for two years. The sections were made by the aid of a microtome, and under water; were stained, and mounted permanently in dammar. Dr. Denny received a vote of thanks from the Association.

#### EVENING SESSION.

The Association met at 8.30 P. M.

Dr. Putnam advocated the use of the gas-cautery for superficial counter-irritation, and showed an instrument which had been modified from an English design. It consisted of a compound blow-pipe with an appropriate handle, to the end of which a small platinum cup, fastened by three stout platinum wires to a brass collar could be attached. A long and fine rubber tube ending in a brass tip for attachment to an ordinary fish-tail burner, carries the gas; a steady stream of air is pumped in by the aid of a rubber hand-bulb and a receiver of very distensible rubber, in



fact, an ordinary toy balloon covered by a good-sized bag of netted twine. The advantage of the instrument over others, consists in its adaptability to almost any place where it is desirable to use the cautery, and the ease with which almost a white heat can be maintained for any length of time.

Dr. Putnam also spoke of the advantage of having the galvanometers which are used for electro-therapeutic purposes, tested, as is done in physical laboratories, in such a way that it could be seen at once by reference to a table or a geometric curve, exactly what strength of current corresponded to any given deviation, and the result expressed in terms of the standard units of quantity—(Volts, Webers, etc). At present it is impossible for different observers to speak in comprehensible terms of the strength of the current which they employ, because the constants of their respective galvanometers are unknown to each other, and the degrees of resistance (relative dryness, etc.,) of the patient's skin cannot be indicated. For practical purposes, great precision is hardly required; but in stating the results of scientific investigations the reverse is often true. In order to have galvanometers graduated in the way designated, it would be only necessary to have them compared with a standard galvanometer, such as exists in all physical laboratories, whose constant bar has been determined experimentally, or by calculation.

The first paper was read by Dr. N. B. Emerson, of New York, and entitled:

#### CLINICAL STUDY OF TETANY.

The paper consisted of the clinical histories of two cases, together with quotations from various authorities regarding the identity of the affection. Dr. Emerson believed that the disease was entitled to a distinct recognition. Both cases recovered.

Dr. Webber related the history of a case in which recovery took place.

Dr. Miles mentioned a case in which he had mistaken the symptoms, due to a probable thickening of the membranes of the cord produced by syphilis, for those of tetany. The patient made a good recovery under anti-syphilitic treatment.

Dr. Jewell made inquiry concerning the pathology of the affection.

Dr. Emerson expressed the opinion that the disease was simply reflex and functional.

Dr. Mary Putnam-Jacobi, of New York, upon invitation, participated in the discussion, and referred to two cases, which came under her observation in one of the clinics in Paris, and in which the symptoms bore a striking resemblance to each other, although one patient, a child two years of age was admitted for spasms of the glottis, and recovered, and the other suffered from what, at the autopsy, proved to be cerebro-spinal meningitis. In the first case, the symptoms of tetany disappeared under the use of musk.

Dr. Seguin referred to two cases, one of tetany, and the other hysteria, in which the symptoms bore a very close resemblance. In the hysterical case, the typical cone-shaped hand was present; and in the case of tetany, at first, no spasm had been seen, but in a short time spasm was developed, and the diagnosis made clear.

Dr. Hamilton referred to a case in which spasm of the muscles was present, and it seemed impossible to make a diagnosis until an anæsthetic was administered, when the symptoms disappeared.

Dr. Miles related the history of a case in which extreme spasm of the muscles of the extremities and trunk was developed, in consequence of pressure upon the spinal cord, produced by an abscess upon the shoulder rupturing into the spinal canal outside of the dura mater. The spinal cord, when removed, seemed healthy. There were no bed-sores.

Dr. Putnam inquired whether tetany became persistent, as hysteric contractions sometimes did?

Dr. Emerson replied that he had not observed any such cases.

Dr. Swezey, upon invitation, spoke concerning the occurrence of tetanus upon Long Island, and knew it to be a fact that punctured wounds were exceedingly liable in that locality, especially upon the eastern portion of the Island, to be followed by tetanus.

Dr. Seguin was of the opinion that

SPONTANEOUS TETANUS

occurred upon Long Island, and

Dr. T. A. McBride, of New York, alluded to cases of spontaneous tetanus occurring in that locality, and mentioned by Prof. T. G. Thomas, of the College of Physicians and Surgeons.

Dr. Miles referred to the spontaneous occurrence of tetanus among the blacks in South Carolina; and

Dr. Dupuy spoke of the very frequent occurrence of the affection in Mauritius.

Dr. Putnam Jacobi referred to two cases mentioned by Bernard.

Dr. Putnam, of Boston, reported the results of his observation in the use of

*CARBONIC ACID GAS IN THE TREATMENT OF HEADACHE.*

The result was negative in eight out of nine cases:

Dr. Dupuy referred to the experiments by Brown-Sequard and himself, which led to the recommendation of the gas for the cure of headache. The cure, it was supposed, was effected by reflex action, the carbonic acid gas irritating the pharyngeal and laryngeal mucous membrane. In his experience, cure had been effected for the time being, except in a few instances. In the cases in which failure followed its use, the hysterical element was present.

*FRIDAY, JUNE 9—THIRD DAY.*

*Afternoon Session.*

Present—Drs. Jewell, Hammond, Miles, Cross, Hamilton, Putnam, Mason, McBride, Kinnicutt, Beard, Lente, Shaw, Arnold, Seguin.

The Association was called to order by the President, Dr. Jewell, and immediately went into executive session.

Dr. Eugene Dupuy, of New York, was elected an active member of the Association.

Dr. Putnam exhibited microscopic specimens of syphilitic artery of the cortex cerebri, and of sclerosis of the posterior columns of the spinal cord, which were prepared by Dr. Edes, of Boston.

Dr. Hammond presented a case of cross paralysis, which, with three others, he proposed to make the basis of a paper to be submitted to the Council.

The Association granted permission, and also granted permission to submit a paper upon



CEREBRO-CARDIAC DERANGEMENT, PROBABLY IDENTICAL WITH  
CHLOROSIS.

The first paper was read by

Dr. A. McLane Hamilton, of New York, and entitled

## OBSERVATIONS UPON CHRONIC EPILEPSY.

In the analysis which the doctor gave of twenty-seven cases that had fallen under his observation, it was found that some had depended upon uterine disease, others upon masturbation, still others upon the presence of intestinal parasites, and in a certain number of instances syphilitic poison seemed to be the cause, etc., etc.

The paper gave rise to discussion, chiefly with reference to treatment. The first indication was to remove the cause, if it could be ascertained. To allay arethism and reduce the susceptibility of the medulla, and to administer some general nerve sedative, were the leading secondary indications. In those cases in which there was a tendency to anaemia, the bromides did harm.

The doctor claimed that not more than one drachm of either of the bromides should be administered during twenty-four hours.

In those cases in which there was present a tendency to hyperaemia, ergot in large doses was recommended.

Attacks of *petit mal* could be cut short by hypodermic injections of atropine.

Dr. Hamilton regarded digitalis as one of the most important adjuvants we possessed in the treatment of epilepsy.

Nitrite of amyl was regarded as an agent which could afford temporary relief only, and was chiefly serviceable in those cases in which a succession of fits occurred. The doctor regarded nitro-glycerine as an excellent prophylactic. He had used it in solution containing about one-quarter of a drop to five minims of alcohol, and had found that it produced almost an exact effect with nitrite of amyl, but the effects had been more permanent. The principal point in the treatment which gave rise to discussion, was that relating to the use of small doses of the bromides.

Dr. Hammond did not believe that small doses were of any avail whatever, but that that large doses should be administered

and repeated, so as to develop the bromic cachexia as quickly as possible. One hundred and sixty grains a day had been given before any effect upon the paroxysms was produced. Administer the bromides, gradually increasing the size of the dose, until the normal excitability of the pharynx was diminished; and unless that condition was present, it was evident that a sufficient quantity of the drug had not been given. In certain cases in which the paroxysms were kept up by force of habit, nitrite of amyl might effect a cure, but, as a rule, its effect was only temporary.

Dr. Miles regarded it as necessary that bromism should be produced, and it might be necessary to administer more than grs. xl. three or four times a day.

Dr. Hamilton thought bromism injurious, because it depressed the system. He regarded grs. xx. t. i. d. as sufficient; if combined with ergot, which contracted the arterioles.

Dr. Putnam inquired whether the bromides produced their good effect in epilepsy, by contracting the arterioles, or by producing some direct effect upon the nervous centres? Furthermore, was it known that the amount of blood in the medulla was diminished under their influence?

Dr. Cross, of New York, believed that complete bromism should be produced, and that the patient should be in that condition.

Dr. Seguin remarked that he never gave less than a drachm of the bromide within twenty-four hours, and frequently as much as grs. 120, or even 160. His rule was to administer at bed-time the maximum dose for the day, because the excitability of the spinal axis was greatest at night. A degree of bromism should be produced in the patient, and the advanced symptoms of bromism could not be developed by administering only sixty grains a day.

Dr. Seguin habitually administered other remedies in combination with the bromides. He used ergot considerably, and in doses of one drachm of the fluid extract three or four times a day. Strychnia he regarded as almost a specific against the motor symptoms of the bromides. Arsenic would diminish the acne of bromism, and in many cases prevent its occurrence. Tonics of various kinds, especially chalybeates, should

be given regularly, in order that the bromides might be better borne.

Dr. Hamilton remarked that there were cases in which large doses of the bromides could be used with benefit for a week or so, but that he recommended the regular administration of only grs. xx. t. i. d., and the occasional use of large doses—even to sixty grains.

Dr. F. P. Kinnicutt remarked that his experience was entirely in accord with Dr. Seguin's, especially with reference to the size of the doses of bromides and the combination with other remedies.

Dr. Jewell remarked that it seemed to him the fits occurred just in proportion as the patient became fatigued. That was not invariably the case, but quite regularly so. He had therefore given small doses of the bromides during the fore part of the day, a large dose after dinner, and the next large one at bed-time. He regarded the production of a certain amount of anæsthesia of the pharyngeal and laryngeal mucous membrane as important.

Dr. Cross raised the question, why administer a large dose of the bromides at bed-time if bromism was all that was necessary?

Dr. Seguin remarked that the effect expected from a large dose of the drug at bed-time was temporary and immediate, and at the same time it contributed to the more permanent influence arising from a certain degree of bromism. If the fit occurred in the day-time, the time of administering the large dose might be changed.

Dr. Miles regarded it as important to find out when the attack, as a rule, occurred, and apply the remedy accordingly.

The paper was further discussed by Drs. Hamilton, Hammond, Dupuy, and Seguin; and in the course of the discussion attention was called to the *nitrite of amyl bulbs* devised by Dr. T. A. McBride.

The next subject brought before the Association was

#### REGIONAL CEREBRAL DIAGNOSIS,

By Dr. F. T. Miles, of Baltimore.

The case which Dr. Miles narrated was interesting, from the fact that the injury of the brain in a particular locality was

connected with the exact symptoms observed before the death of the patient.

The brain injury in this instance occurred in an eminent lawyer who, at the time symptoms were first developed, was cross-examining a witness. A fac-simile of his handwriting was exhibited, and it could be seen exactly where irregularities in outline of letters began to show themselves.

From that point the chirography became more and more illegible, until finally a mere blot was made. The gentleman rising to address the court, it was perceived that something was wrong with him, and he was removed from the room. He became semi-comatose, had complete right hemiplegia, the right side of the face was completely paralysed, and *ptosis* of the left eye was strongly marked. The tongue was not paralyzed, but the patient was unable to thrust it out, and there was no paralysis of the muscles of deglutition. There was some anæsthesia upon the affected side. The patient answered all questions affirmatively. The eyes could be made to follow the finger only as far as the median line, when they would stop, and the patient would carry them over to the other side. The case terminated in the usual deep coma and death. Dr. Miles had diagnosed *crural hemorrhage*, producing *ptosis*, with paralysis upon the opposite side. At the autopsy a mass of blood which had so flattened the convolutions, that it could be easily seen beneath the surface, was found in the central portion of the middle lobe of the brain upon the left side. When the sac was opened, the first blood that escaped was fluid; then came a jelly-like mass; and, lastly, a hard clot, not easily broken up, which was probably the original clot that gave rise to the first symptoms. The clot did not break through into the ventricles, but remained enclosed in the substance of the third lobe, and made pressure upon the outer part of the left crus cerebri and third nerve.

Dr. Miles referred incidentally to a case in which an abscess occupied by the same region occupied by the original clot in the case being described, but in which there was not a shadow of motor paralysis. In the present instance, the autopsy proved that the diagnosis was incorrect, because there was no hemorrhage into the crus. The

symptoms, therefore, were explained by the double influence produced by the destruction of brain substance, and pressure upon the surrounding parts; and that it was the pressure upon the crus and the third nerve that gave rise to the eye symptoms. Dr. Miles demonstrated his remarks by means of finely executed models of the human brain.

Dr. Jewell remarked that the fact of the symptoms in Dr. Miles' case being due to pressure produced upon parts outside of the real lesion, showed how easily mistakes could be made with reference to diagnosis and prognosis.

Dr. Hammond inquired with reference to dilatation of the pupil and strabismus.

Dr. Miles replied that there was no special dilatation of the pupils, and that he did not observe any strabismus.

Dr. Hammond, from these facts, was inclined to regard the ptosis as entirely independent of intra-cranial trouble. He did not regard the case as one of cross-paralysis, but that the ptosis depended upon some orbital lesion.

Dr. Seguin directed attention to one symptom in Dr. Miles' case which would cast a doubt upon the diagnosis of the affection of the crus—namely, the deviation of the eyes. The fact that the patient could not be induced to turn the eye to the right beyond the median line and tended always to look to the left was an indication of lesion of the left hemisphere, and not of crural lesion. The case did not present the classic symptoms of palsy from lesion of the crus cerebri. Had he seen the patient he would have probably thought of a double lesion.

Dr. Eugene Dupuy, of New York, read a paper upon localization of motor centres, which may be summarized in the following propositions:

The cortical substance of the brain is not excitable by any means, either physical, chemical, or mechanical.

The effect of electricity, when applied to the surface of the brain, is not localized, but diffused; and the motor results obtained when electricity is used are not due to the direct action of the irritant, but are altogether reflex.

No part of the brain substance is excitable except the nerve fibres, and there is no way by which nerve cells can

be excited except by exciting the nerve fibres with which they are connected.

When electricity was applied to the cortex, motion might take place, but it was not the nerve cells themselves which were put into action, as proved by experiments. The cranium of an animal was laid open, a point made sure, which, when irritated, gave rise to certain motions, and then the pia-mater was rendered opaque by means of hot iron, but motion followed the application of an irritant, the same as before. The wound was then allowed to close, and at the end of a month, the electrodes were applied over the dura-mater covering the same spot, and motion was produced on the same side of the body. The dura-mater was then divided, and the white patch in the pia-mater exposed, and then, when the electrodes were applied, slight motion followed, but not so marked as before. The surface of the part was then dried by means of blotting paper, and the electrodes applied, but no motion followed. The sclerosed patch of pia-mater was then removed, and the surface dried, but no motion followed the application of the electrodes.

If motion did not take place in the second series of experiments, it was not from lack of cortical substance, because the fibres below were found to be healthy when examined with the microscope.

It was then clear that the motion was not due to disturbance of the nerve cells, nor to disturbance of the nerve fibres, except those in close relation with the blood-vessels. The Doctor then undertook the examination of the structure of the brain tissue and pia-mater; and he had found that the only points in the cortical substance where motion could be produced by the application of an irritant, were those supplied by vessels which came from the pia-mater, and dipped deep into the substance of the brain, and had nerve fibres accompanying them. He believed, therefore, that there was a nervous connection between the pia-mater and the fibres from the spinal cord which was continuous, and that motion was produced through that connection when an irritant was applied to the pia-mater.

The paper being open for discussion,

Dr. J. W. S. Arnold, of New York, asked Dr. Dupuy what current he employed to stimulate the brain; the induced or the constant current, or the primary or secondary induced current?

Dr. Dupuy replied, that it made no difference with regard to the current employed.

Dr. Arnold remarked, that it was an important point to be decided; and that he was convinced the diffusion of the induced current was so great that its action could not be limited, whereas the constant current could be limited, and the exact point from which the action came could be determined.

He had also been able, by means of the constant current, to decompose brain tissue to such an extent that motion in certain muscles or groups of muscles, in response to the electrical irritant, could not be reproduced; and that when such tissue had been examined microscopically, the structure had been found to be entirely changed. He had also found that when certain points in the brain, which seemed to have presiding power over definite sets of muscles, were destroyed by means of the constant current, motion could no longer be produced in response to the electrical irritant.

Those were the arguments he brought against Dr. Dupuy's theory, and in favor of localization of motor centres. Dr. Arnold inquired of Dr. Dupuy, whether the motion was due to the irritant effect produced upon the involuntary muscular fibres in the coats of the arteries?

Dr. Dupuy replied, that it was.

Dr. Arnold remarked, that the action of involuntary muscular fibres was slow, but that the motions produced in response to an electrical irritant applied to the surface of the brain, were instantaneous.

Dr. Dupuy remarked, that he had not seen movements follow immediately upon the application of the irritant.

Dr. Mason remarked, that he had made enough experiments on diffusion to satisfy himself that, practically, there was no difference in this respect, between the current employed by Ferrier and that used by Hitzig. The electrical sensation received on the tongue of the experimenters, was perhaps the best off-bound index of intensity that we possess, but it is im-

portant to distinguish between the burning felt during the circuit's closure, and the sensation at the moment of closing the circuit.

The fact, that the current from the second coil of DuBois-Reymond's apparatus, is one of higher tension than a simple Voltaic current, does not favor the view that the former is more diffusible than the latter, for from the laws of traveling currents, we know that the less the resistance in the main circuit the less the diffusion in the accessory circuits; and may therefore conclude, that if two shocks, the one which the better overcomes resistance in the main circuit, is the one which is the less diffusible outside of it. The cause of the exceeding diffusibility of static shocks, is due to the fact that it is difficult here to avoid the phenomenon of discharge; the whole brain of the animal becoming charged and discharged at each shock, however feeble.

His experiments were made on masses of moistened clay and simple water. He employed the galvanoscopic frog as a test of diffusion, fixing the leg on the "universal support" of DuBois-Reymond, and allowing a few mm. of the nerve to come in contact with the clay or to dip into the water. It is very easy in this way to convince one's self that Ferrier's current, so far as diffusion is concerned, is as good as Hitzig's, and that static electricity ought never to be used in experiments on the brain.

Dr. Arnold remarked, that he had records of movements following immediately upon the application of the irritant; and not only so, but that he had measured the difference in time which elapsed before motion was produced when the irritant was applied to the involuntary muscular fibres of the blood-vessels in the brain, and when it was applied to the cortical substance and gave rise to motion in the voluntary muscles, in the limbs of the animal; and that there was a very appreciable difference in the length of time which elapsed before motion was produced in the two instances. In all his experiments, also, the brain surface was kept free from moisture.

Dr. Jewell thought, that although Dr. Dupuy's paper was interesting with respect to the question of the mechanism by which the excitation led to the contraction, it did not militate



greatly against the doctrine of localization of function, but was simply another method of explaining how the contraction was brought about. If the contraction was due to a modification in the nutrition of the part irritated, that view was just as favorable as any other to the doctrine of localization. The theory advanced by Dr. Dupuy, for aught he knew, might be correct; but it did not, in his opinion, affect the doctrine of localization of function at all, but was simply another way of explaining the phenomenon of voluntary muscular contraction. He was strongly inclined to believe in the doctrine of localization of function.

At the close of the discussion, Dr. Hammond nominated Dr. J. C. Peters for membership, subject to the action of the Association at its next annual session.

The Association was called to order by the President; and, under the head of Miscellaneous Business, it was resolved to hold the next annual session in the city of New York, commencing on the first Wednesday of June, 1877.

The following papers were then read by title, and referred to the Council:

The Development of the Nervous Tissue in the Human Embryo, by Dr. H. D. Schmidt, of New Orleans. Dr. Schmidt has transmitted the paper to the Association. It is an exhaustive essay upon the subject indicated, embodying the results of the author's original work. Accompanying the MSS. is an album of beautiful drawings illustrating the histology of the subject.

Contributions to the pathology of the Fronto-Parietal Region of the Cerebrum, with Case, by Dr. T. A. McBride, of New York.

Post-Diphtheritic Paralysis, by Dr. J. J. Putnam, of Boston.

On Brain Tumor without choked Disc, by Dr. E. C. Seguin, of New York.

On Tumor of the Pons, by Dr. J. C. Shaw, of Brooklyn.

On Tumor at the base of the Brain, by Dr. E. R. Hun, of Albany.

Dr. T. B. M. Cross, of New York, then read a paper

ON THE RETINAL CIRCULATION IN EPILEPSY.

The paper contained the opinions of various observers upon

the condition of the intra-ocular circulation during epilepsy, and also the results of the Doctor's own observations, made in 95 cases. His conclusions were mainly as follows:—In a large proportion of cases there was no change in the intra-ocular circulation in the interval between the paroxysms, and no alteration of any of the structures of the eye. In some cases there was an increased or diminished amount of blood in the retinal vessels but such change was purely accidental, and not because of the epileptic paroxysm. There might be congestion of the retinal vessels after a fit, but it soon disappeared, whether the fit was nocturnal or diurnal. When anæmia was present, satisfactory results did not follow the use of remedies which were said to increase the amount of blood in the brain.

The papers were discussed by Drs. Putnam, Dupuy, Hamilton, Hammond, and Putnam-Jacobi.

Dr. Putnam raised the question whether the bromide of potassium acted through the blood-vessels or nervous centres, and whether, if contraction of the blood-vessels could be produced in any other way, the same effect would be produced as by administering the bromides?

Dr. Hammond gave a number of reasons for believing that the bromides diminished the quantity of blood in the brain.

Dr. Putnam-Jacobi remarked that, with regard to the question whether the effect produced by the bromides was directly upon the molecular elements of the nerve cells, or first upon the blood-vessels, some light, perhaps, might be shed by the fact of the occurrence of acne while the bromide was being administered. Eruptions similar to acne, as well as acne itself, were developed under conditions closely resembling those present when the bromic cachexia was developed, *i. e.*, after recovery from long-sustained nervous excitement followed by nervous exhaustion. Several cases were cited in which cutaneous eruptions were manifestly of nervous origin, and also cases in which cutaneous eruptions had followed allaying of the excitability of the nervous elements. Might not the occurrence of acne after administration of the bromides, which allays the erethism of the nervous system, be evidence that the effect produced was directly upon nerve elements; and, in

addition, that they produced effects upon the blood-vessels?

Dr. Hamilton remarked that he had seen bromic acne fully developed, and the fits continued as violently as at the beginning.

Dr. Putnam, of Boston, exhibited a  
SECTION OF THE SPINAL COLUMN AND OF THE SPINAL CORD REMOVED FROM THE BODY OF A BOY WHO HAD SUFFERED FROM FRACTURE AND DISLOCATION AT THE TWELFTH DORSAL VERTEBRA.

The last paper was read by Dr. Allan McLane Hamilton, of New York, and consisted of clinical notes upon two cases of  
LOCOMOTOR ATAXIA ASSOCIATED WITH THE GENERAL PARALYSIS OF  
THE INSANE.

The close of the session having been reached, Dr. Hammond moved a vote of thanks to the President for the uniform courtesy he had manifested while presiding over the deliberations of the Association, which was unanimously carried.

Dr. Putnam moved a vote of thanks to Dr. W. M. Carpenter, of the New York MEDICAL RECORD, for his assistance in making the notes upon papers and the discussions, which was unanimously carried. The President, Dr. Jewell, congratulated the Association upon the success which had characterized the present meeting, and spoke for a steadily increasing zeal and activity in its studies and investigations.

The Association then adjourned, to meet in the city of New York on the first Wednesday of June, 1877.

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